and diminishing the velocity which would otherwise be found there."

Then follow the determinations of Boileau and of Bazin, from which it may be inferred "that the ratio at which the maximum velocity is found to the whole depth ranges from zero to 0.2, except in some artificial channels, where it reached 0.35. The Mississippi experiments give different results, and Bazin inclines to believe that the method of experimenting was untrustworthy. The ratio is greatest in artificial channels with smooth bottoms, and least in natural streams with rough bottoms.

It is difficult to understand what force could cause the portions of water retarded by the sides or bottom to spread themselves with constant uniformity over the unimpeded current flowing below the surface in mid-stream, and especially how the portions retarded by the bottom could rise up through or pass around the more rapid portions above them. But the phenomenon becomes very simple if we suppose that each molecule of the water has its own proper motion, governed by well-known mechanical laws. The impetus to the motion is determined by the pressure, and the actual motion is necessarily the resultant of the difference between the pressure and the resistance. If there were no resistance to the flow of the stream, there would be constant acceleration of motion from top to bottom, just as there is in jets from the side of a vessel, the flow from each being determined by the pressure above it. But in a flowing stream there is great resistance from the sides and bottom, the resistance from the bottom necessarily increasing with the pressure, and this resistance which the molecules receive from the bottom is transmitted, just as pressure is from above, to the molecules adjacent to them. At the depth where the impetus to motion by the pressure from above comes into equipoise with the resistance to motion from below, there ought to be, as there is in fact, the greatest velocity of flow. The resistance from the bottom remains practically constant at any given place in the stream. Wind blowing up-stream increases the pressure by holding back the surface molecules; hence this increase of pressure, the resistance remaining constant, causes the level of maximum velocity to descend. On the other hand, when the wind blows down-stream there is a diminution of pressure, because the surface molecules are pushed forwards in the direction of their movement; hence this diminution of pressure, the resistance still remaining constant, causes the level of maximum velocity to ascend. When the flow is through a round pipe entirely filled with water, and under such pressure that the influence of gravity on the stream itself may be disregarded, it is obvious that the maximum velocity is through the centre of the pipe; the pressure is uniform in all parts of a cross-section of the pipe, and the resistance from friction against the pipe is likewise uniform in all directions from the centre.

It is not necessary to seek further evidence of molecular motion in other phenomena of hydraulics. The evidence is manifest in all the phenomena that I have examined; and the motion is not only consistent with the facts, but the hypothesis of its existence clears up many things which without it are obscure. The explanation which it furnishes of the phenomena of wave motion is especially interesting, but the subject is too large for consideration in this paper.

It seems to me, therefore, that, without further illustration, we may assume as determined that, in all flowing, the particles or molecules constituting the body in which the phenomenon occurs, whether visible or invisible, have each its own proper motion, determined by the forces and resistances to which it is subjected, and that the molar motion is made up of the aggregation of these molecular or particle-motions, — and in this consists the specific difference between flowing and sliding.

This determination is evidently of theoretical importance in hydromechanics and in pneumatics, for the law must apply to the flowing of gas as well as to the flow of liquids, and it may lead to other determinations of great practical value in one or both of these sciences. But since Mr. Crookes has put the molecules of residual gas, in the bulb of the radiometer and in his tubes, to doing mechanical work, the basis has been laid for the development of the science of molecular mechanics, and it is in this new field that this determination has its greatest importance.

The eyes of scientists are being directed to what we might call the small end of nature, and we are discovering that microbes, bacilli, bacteria, etc., are of more importance to mankind than the cedars of Lebanon, or the beasts which roamed beneath them, or the birds which sought shelter in their branches. So in this new science of molecular mechanics, the way to which has been opened up to us by Mr. Crookes's researches, we have the promise of additions to scientific knowledge more important even than the magnificent results which followed the application of mechanical laws to the movements of the celestial bodies.

DANIEL S. TROY.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

Rain-Making.

IN the issue of *Science* of Aug. 28 there appears a communication from Professor H. A. Hazen attacking the artificial rain theory, to some points in which I ask the privilege of making reply.

Professor Hazen commences by saying that "ever since the time of Plutarch the idea has been prevalent that great battles are invariably followed by rain." Now, I would ask where Professor Hazen gets his authority for this broad and sweeping statement? In what writings, following those of Plutarch, does he find any reference to the matter up to the time of Benvenuto Cellini, who is said to have written that a discharge of artillery affected meteorological conditions? Plutarch lived in the first century of the Christian era, Cellini lived in the sixteenth century. Here is a great gap of about fifteen hundred years, and if there is any evidence that the idea prevailed, during that time, that battles caused rain, I challenge my critic to produce it.

A great many writers besides Professor Hazen have brought forward the statement of Plutarch relative to rains following battles as an argument against the concussion theory of rain-production, and some appear to think the argument quite unanswerable. It is, however, very easily disposed of, for the notion referred to by Plutarch was an entirely different matter from that which, so far as we know, did not come into notice until fifteen hundred years later. It was wholly different, in that it did not relate to rains immediately following battles. The only place in which Plutarch mentions the subject is in his life of Marius, in speaking of the defeat of the Ambrones by the Romans. The rains which he says followed that battle did not occur until the winter following. And in mentioning the subject in a general way in connection with this one specific instance, the whole tenor of what he says conveys the idea that the rains he referred to did not occur until a considerable time after the battles, nor until the bodies of the slain had putrified. To give what he says other meaning is to make his attempted explanation of the cause of the rains wholly

inapplicable. How absurd, then, to claim that the ancients had the same notion in regard to rains following battles as that which prevails at the present day. We might well question, indeed, whether there was any such idea prevailing among the ancients as that to which Plutarch alludes, as a single and unsupported statement by one writer alone is not very conclusive evidence; but admitting that he may have spoken advisedly on the subject, it is plain that it was not a "common thought" with that of the moderns, and all reasoning against the concussion theory based on it must fall to the ground.

The second point in Professor Hazen's article to which I wish to refer is the wholly unwarranted assumption that all, or nearly all, the battles of our late war which I have not shown in "War and the Weather" to have been followed by rain were not so followed. On a par with this is the violence he does to history in assuming that the 2,200 battles which he says were fought during that war were, on an average, as severe as the 158 mentioned in my book. The greater part of the 2,042 which he says I do not mention could have been nothing more than skirmishes. The most remarkable thing about Professor Hazen's article is that although he has read my book he pays no attention to any of the explanations or arguments I make. I explain the difficulty, from want of records, of getting reliable information in regard to the weather following the land battles that were fought, and he coolly proceeds to count all those not proved to have been followed by rain as belonging to the other side. I give a reason why we should not expect skirmishes to produce rain, and he counts them all in as if one such, not followed by rain, furnished as good evidence against the theory as a great battle followed by heavy rain furnishes for it. By this cheap method of figuring he makes out that only seven per cent of the battles were followed by rain. What weight has such an argument against the fact that all the great historic battles of the war, so far as reliable information can be obtained, were followed by heavy rains?

Professor Hazen argues that the influence of explosions could not extend twenty-four hours, for the reason that the current subjected to it "is borne along at the rate of 20, and, in the higher strata, at 30, 40, 50, and more, miles per hour, so that the specific influence from them will be carried at least 500 miles away in twenty-four hours." Now the learned professor cannot be sure of his ground here unless his knowledge of all atmospheric movements and of processes in the formation of storm centres is infallible. It is generally understood that our Weather Bureau claims such infallibility, though Professor Hazen in another part of his article seems to disclaim it, and though some of the unscientific laity are inclined to believe that the whole orthodox theory of rain-formation will yet have to be remodelled.

Professor Hazen does not think my explanation of the point under consideration worthy of notice, as he does not refer to it. This explanation is as follows. The storm centre may remain stationary over the place where the firing takes place until the storm is fully established, because it is caused by the mingling of two currents of air flowing in nearly opposite directions. At the commencement, the new action set up is confined to the upper stratum of the lower current and the lower stratum of the upper current. These, mingling together, set up a rotary motion, but as a whole, the air partaking of this motion moves neither very far east nor very far west, being acted on by opposing forces, one tending to carry it eastward and the other westward. When, however, large enough volumes of air become involved in the motion to produce rain, the storm will move eastward along with the warm current. As this is not orthodox philosophy as held by the scientists of the Weather Bureau, Professor Hazen will have none of it. But perhaps he will remember cases in which storm centres have lingered long in one place, and, if so, this fact alone furnishes a sufficient argument in refutation of his own

There is only one other point in Professor Hazen's article that I wish to notice, and that is this: he says, "One thing seems very evident, that absolutely no rain can be obtained out of a dry atmosphere." This is an old argument the extreme tenuity of which I have often shown. Professor Hazen well knows how I have met it by showing that there are probably at all times sufficient quantities of aqueous vapor flowing above us in air currents to make rain. He cannot refute my argument on this point, nor I believe, show that there is anything unreasonable in it, therefore he very wisely ignores it. My argument is based on the absolutely certain fact that as much water must come to us from the ocean as runs into the ocean from our rivers, and on the further fact, demonstrated by Professor M. F. Maury, that most of the vapor that forms our rains comes to us from the Pacific Ocean. Coming from the Pacific, it necessarily comes in air currents which flow above the mountains and high above the arid regions of the West. Meteorologists will come nearer a solution of the problem of rain-production when they recognize the fact that it is not the moisture in the lower air east of those mountains and arid districts that gives us our rains, but that it is the rains formed mainly by the condensation of the vapor from the Pacific that cause the moisture.

EDWARD POWERS.

Delavan, Wis., Sept. 26.

BOOK-REVIEWS.

Schliemann's Excavations. By DR. C. SCHUCHHARDT. Trans. from the German by Eugénie Sellers. New York, Macmillan. $4.

THE object of this work is to give a succinct account of Dr. Schliemann's discoveries, sufficient for most students of the subject, and presenting the net results in a single volume. The reports heretofore made of the excavations, chiefly by Dr. Schliemann himself, are contained in several different books published at intervals, none of which contains a complete account of the whole work, so that a good summary was much needed; and such a summary Dr. Schuchhardt, with the approval of Schliemann himself, has here given us. He has also taken account of the discoveries that have been made by others, especially those of the Greek Archæological Society, while Drs. Schliemann and Dörpfeld have given in an appendix reports of their excavations at Hissarlik last year; so that we get a complete account of all that has been done. Mr. Walter Leaf contributes an introduction in which he discusses certain points of interest, expressing in some cases somewhat different views from those of Dr. Schuchhardt. Dr. Schliemann's work was so emphatically the result of his own personality, and his life was in itself so interesting, that Dr. Schuchhardt very properly begins his volume with a biographical sketch. Schliemann was the son of a clergyman, and received excellent schooling in early boyhood; but, owing to misfortunes in the family, he was obliged to leave school and go to work to earn his living. For several years his life was hard; but at last a firm in Amsterdam detected his commercial abilities, and from that time his advancement was rapid. The foundation of his large fortune seems to have been laid in Russia during the Crimean war; but it was not until several years later that he was able to retire from business with a fortune sufficient to carry on the archæological researches which had been the dream of his life. The first sod was turned at Hissarlik in 1870, and, as the excavations were continued with some interruptions until the great explorer's death last year, they covered a period of twenty years.

Of the importance of the work thus done there can be no doubt; it was, as Mr. Leaf remarks, nothing less than the creation of prehistoric Greek archæology. Before Schliemann's excavations began, most scholars doubted the story of the Trojan war, maintaining that it was a poetic fiction and that the personages represented in the "Iliad" and "Odyssey" were mythical, and there was great uncertainty as to the site of Troy itself. Dr. Schliemann has now uncovered the site of Troy just where Greek tradition uniformly placed it; and, as the ruins show that the city was destroyed by fire, its reduction by siege is highly probable. Thus far only the citadel has been excavated; but the massiveness of its walls prove that it must have been the nucleus of a large and powerful city, though the utensils and ornaments that have been found indicate a lower stage of civilization than that of the prehistoric cities on the European side of the sea.

It is at these last-named cities, indeed, and especially at Mycenæ and Tiryns, that the most important discoveries have been made. Tiryns, which stood nearest the sea, was first excavated, and here Schliemann first had the assistance of Dr. W. Dörpfeld,